Fourth, the same when the blowing was finished, but before the spiegeleisen was added.

Fifth, the finished steel when poured into the ingot moulds.

	1	2	3	4	5
Combined carbon	1·000	3·040	1·640	0·190	0·370
Graphite . . .	2·570	trace	trace	trace	trace
Silicon	2·260	0·955	0·470	trace	trace
Sulphur	0·107	0·091	0·098	0·093	0·090
Phosphorus . .	0·073	0·070	0·070	0·070	0·059
Manganese . . .	0·410	trace	trace	trace	0·540

We shall now be able to understand the changes I have described as occurring in the flame. Before the full combustion of the carbon can commence, there is about 2½ per cent. of silicon to be converted into silicic acid. In a charge of 6 tons this amounts to 3 cwt. For the complete combustion of this, nearly 3½ cwt. of oxygen, or about 14 cwt. of atmospheric air, is necessary. My explanation of the smaller and less brilliant flame that at first roars from the mouth of the converter is that it is mainly a silicon flame, mingled, however, with a small proportion of carbon flame; that the amount of silicon combustion goes on diminishing, and in a proportionate degree the carbon combustion increases, as the demand of the silicon upon the oxygen of the blast diminishes in consequence of its less abundant diffusion among the melted iron.

I shall not be surprised if this explanation is controverted, as in offering it I fly in the face of the spectroscope, which has made such glorious conquests that modern philosophers are disposed to trust it as implicitly as successful soldiers rely upon the general who has led them continually to victory; but without failing in due deference to those who are more skilful than I am in the use of this instrument, I am satisfied that in this and other cases where the question has been to determine the presence or absence of the *metalloids*, the *negative* replies of the spectroscope have been too hastily accepted. Prof. Roscoe, who devoted a considerable amount of time and labour to the spectroscopic examination of the Bessemer flame, says, "Those who are practically engaged in working this process would like spectrum analysis to do a great deal more; they would like to be told whether there is any sulphur, phosphorus, or silicon in their steel; questions which, unfortunately, at present spectrum analysis cannot answer, for this very good reason, that these substances do not appear at all as gases in the flame, but that they either remain unvolatilised in the molten metal, or swim on its surface in the slag of the ore; and, consequently, the lines of these bodies are not seen in the spectrum of the flame." Dr. Watts's observations and conclusions accord with those of Prof. Roscoe.

If by the above Prof. Roscoe is to be understood as asserting that no portion of the Bessemer flame at any period of its existence is due to the combustion of silicon, or that silicon is not present in the Bessemer flame, I must very decidedly affirm that such conclusion is erroneous. I do not for a moment question the accuracy of the observations of both Prof. Roscoe and Dr. Watts. I merely maintain that the absence of "the lines" of silicon in the spectrum of the flame does not prove its absence as a constituent in producing such flame, and for the following reasons:—

We know that that the silicon existed in the pig-iron in the proportion already stated, and that although a very small quantity of that which ordinary analysis detects may have existed as entangled silicate in the pig, and another small portion is of course oxidised in the cupola, the bulk of it enters the converter as unoxidised silicon, and that it is oxidised and converted into silicic acid during the blow. We also know that silicon when heated in air or oxygen burns brilliantly, and that the product of such combustion when heated with a blast such as that which supplies its oxygen in the Bessemer converter, is sufficiently volatile to form concretions in the throats of furnaces, which have been compared to natural chalcedony. Besides this a large quantity of solid matter is mechanically forced into the flame, and is seen above as a red smoke, which, without the slightest indication of unburnt carbon, is often sufficiently dense to hide the mid-day sun. The greater the quantity of silicon in the pig the more dense is this red smoke, which appears to consist of silicate and peroxide of iron. I maintain, therefore, that silicon is there, and that it must contribute to the luminosity of the flame, though it shows no characteristic "lines" in the spectrum.

Under such circumstances, we have no good *a priori* reasons for looking for the silicon *lines*; a *continuous spectrum* being that which we are theoretically justified in anticipating as the result of such combustion of silicon, and this is exactly what the spectroscope reveals. The spectrum of the Bessemer flame at the commencement and early stages of the blow is of a most uncommunicative continuous character: occasional flickerings and vanishing ghosts of lines and bands come and go with perplexing irregularity; and even the brilliant and ever obtrusive sodium lines do not appear at this stage, but commence with spasmodic flashings across the spectrum at about the period when the elongation and brightening of the flames which I have described is most decidedly taking place. When the flame has reached its maximum of extension and brilliancy, the sodium lines cease their intermittent flashings, and become a steady stream of light, the lithium band appears (though not in every blow), and the whole spectrum becomes striped, but the continuous spectrum still remains as the permanent background.

My general reason for questioning the negative conclusions of the spectroscope in reference to silicon and the other metalloids is, that these bodies usually give a continuous spectrum when, as elements, they combine freely with oxygen, as in direct unrestrained combustion under ordinary pressure in the open air. It appears to me that there is thus presented a broad distinction between the spectra of the metals and of the non-metallic elements, which is of great practical importance, and which has not been sufficiently considered, when conclusions have been based on negative spectroscopic results. I have already, in chapter 13 of "The Fuel of the Sun," referred to the worthlessness of the negative evidence of the spectroscope in reference to the non-existence of the metalloids in the sun, and maintain that "they may all be there though the spectroscope should not detect one of them." The mere fact that nothing but metals (I include hydrogen with these) should have been discovered in the sun is very suggestive.

W. MATTIEU WILLIAMS

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 23.—"On the Thermo-electric action of Metals and Liquids," by George Gore, F.R.S. It is well known that the degree of rapidity with which a metal immersed in an acid, alkaline, or saline liquid is corroded varies considerably with the temperature, and that the speed of corrosion usually increases with the heat; also a few experiments have been published (Gmelin's "Handbook of Chemistry," vol. i. p. 375) showing that changes of electrical state occur in metals under such circumstances; but a further examination of the relations of the temperature and chemical change to the electrical state has not, that I am aware, yet been made.

In an investigation on the development of electric currents by unequally heated metals in liquids (*Phil. Mag.* 1857, vol. xiii. p. 1), I found that hot platinum was electro-negative to cold platinum in liquids of acid reaction, and positive to it in alkaline ones, provided in all cases chemical action was completely or sufficiently excluded. In the present experiments I have endeavoured to ascertain what electrical changes are produced in cases where chemical action more freely occurs, and I have therefore employed not platinum plate, but plates composed of a metal (copper) which is more easily corroded.

The following are the general results arrived at.

The chief fact brought out conspicuously by the experiments with copper dishes is, that in many cases an increase of chemical action produced by heat instead of making the hot metal electro-positive makes it considerably negative.

The results show that hot copper was positive to cold copper in the following liquids:—hydrochloric, hydrocyanic, boracic, and tribasic ortho-phosphoric acids; chloride of copper (weak solution); chloride of cobalt; chloride of manganese; chromic acid; chloride of chromium; sulphate of zinc (weak solution); sulphate of magnesia; chloride of calcium; nitrate and chloride of strontium; chloride of barium; nitrate of sodium (strong solution); chloride, iodide, carbonate, and biborate of sodium; sulphate of sodium (strong solution); tribasic phosphate of sodium; nitrate, chloride, and chlorate of potassium; bromide of potassium (strong solution); iodide of potassium (strong solution); carbonate, acid carbonate, and bichromate of potassium; aqueous ammonia; chloride of ammonium; cyanide and ferrocyanide of potassium; acetate of zinc; and acetate of sodium. And negative in the following ones:—nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids; ferrous sulphate; chloride of copper (strong solution); sulphate of copper; sulphate of zinc (strong solution); nitrate and iodide of sodium (weak solutions); bromide and iodide of potassium (weak solutions); iodate of potassium; chrome alum; nitrate of ammonium; oxalic, acetic, tartaric, and citric acids. The number of liquids in which hot copper was positive was thirty-six, and those in which it was negative was twenty.

In several instances where the hot metal was negative with a weak solution, it became positive with a strong one; for instance, with sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium; but with chloride of copper the reverse occurred. These results may be connected with the fact that in weak neutral solutions the chemical action is generally the most feeble, and therefore interferes the least with the direct influence of the heat in producing electric currents.

The influence of free hydrochloric, hydrocyanic, boracic, ortho-phosphoric, and chromic acids, was to make the hot copper positive; whilst that of nitric, chloric, hydrobromic, hydrofluosilicic, sulphuric, and some of the organic acids was to make it negative.

In consequence probably of the small amount of interference by chemical action in solutions of oxalic, acetic, tartaric, and citric acids, the direct influence of the heat made the copper negative, similar to its influence on platinum, in all acid liquids which do not attack that metal.

The nature of the acid in a salt appears to exert much more influence than that of the base on the direction of the current; for instance, in nearly all chlorides, including those of a considerable variety of bases, hot copper was positive, probably because copper is more readily attacked by acids than by bases.

In all decidedly alkaline liquids the hot copper was positive; this is similar to the behaviour of platinum in such solutions, and is probably due to the same course, viz. the direct influence of the heat, as well as to chemical action.

The results also show that the quantity of the current obtained with any given liquid generally increases with the number of molecules of the substance contained in the solution; in some cases, however, as with sulphuric acid, carbonate of potossium, chloride of ammonium, and acetate of zinc, there was a limit to this increase; and beyond that limit the quantity of the current decreased up to the point of saturation of the liquid.

In the great majority of cases the value of the deflection increased much more rapidly than the strength of the solution, particularly with solutions of sulphate of magnesia, and also of hydrochloric acid and of chloride of sodium, probably because two causes operated, viz. increased strength of solution and diminished resistance; in a very few cases, however, the opposite result took place, as with solutions of chloride and nitrate of strontium.

Inversions of the direction of the deflection by difference of strength of the liquid occurred with solutions of chloride of copper, sulphate of zinc, nitrate, iodide, and sulphate of sodium, bromide, and iodide of potassium.

Irregularities of the amount of deflection were very apt to take place with liquids which gave strong deflections, or which acted much upon the copper plates (for instance, nitric acid), especially if bubbles of air remained under the plates, or the dishes were wetted on their side above the liquid by the solution.

In certain acid liquids, viz., nitric, chloric, hydrobromic, hydrofluosilicic, and sulphuric acids, the hot copper was strongly negative (notwithstanding the chemical action upon it was distinct, and in some cases even strong); this is similar to the electrical behaviour of platinum in such liquids, and may be attributed either to the more direct influence of the heat alone (such as occurs with platinum plates), or to a different influence of the chemical action produced by the heat. Both these causes probably operate in such cases.

It is probable that in all cases where the hot copper was positive in liquids of strongly acid reaction, the positive condition was due to chemical action alone.

With some liquids, especially with solutions of hydrocyanic, boracic, acetic, tartaric, and citric acids, the deflections were very feeble, and the chemical action on the plates not perceptible; whilst with others, such as nitric and chloric acid, solutions of the chlorides of strontium, sodium, potassium, and ammonium, and of carbonate, acid carbonate, and cyanide of potassium, the deflections were considerable, and the chemical action distinct, and in some cases strong. In none of the liquids (except hydrobromic and chromic acids) did the hot plates appear to be *less* stained or corroded than the cold one; probably, in all cases, it was the most corroded, although in some cases the corrosion was not perceptible.

The amount of deflection was not always proportionate to the amount of chemical action; for instance, with solutions of chloride of copper and iodate of potassium there was considerable corrosion, but only feeble currents, probably because the plates became covered with a badly conducting film, whilst with hydrochloric acid, chloride of cobalt, chloride of manganese, and nitrate of potassium, the reverse occurred.

I consider the currents in all these experiments of difference of temperature to be due either (1) to the direct influence of heat, the effect of which is to make the hot copper negative in acid liquids and positive in alkaline ones (see *Phil. Mag.* 1857, vol. xiii. p. 1); (2) to chemical action, which sometimes overpowers the direct influence of heat and reverses the effect; or (3) to both these influences combined. The more ultimate cause, however, of the phenomena in these causes must be sought for in the *molecular movements* produced by heat in the metals and liquids.

The current obtained with copper plates were no doubt influenced in their amounts (if not also in their direction) by the oxidising action of the air upon the liquid and metal at their line of mutual contact, for we know that metals in contact with liquids oxidise much more quickly if oxygen has access to their wet surfaces; and the currents were also influenced by the action of unequal temperature upon the air-contact line, for we know that wet metals oxidise still more rapidly if heat is applied.

General Conclusion.—The electric currents produced by the direct influence of unequal temperature or friction of platinum or copper electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and may be considered as a very delicate test of the kind and amount of temporary molecular movements produced by those causes.

"Further Experiments on the effect of Diet and Exercise on the Elimination of Nitrogen," by E. A. Parkes, M.D., F.R.S.

"Magnetic Observations made during a Voyage to the North of Europe and the Coasts of the Arctic Sea in the Summer of 1870," by Capt. Ivad Belavenetz, I.R.N., Director of the Imperial Magnetic Observatory, Cronstadt.

"On the Mutual Relations of the Apex Cardiograph and the Radial Sphygmograph Trace," by A. H. Garrod, of St. John's College, Cambridge.

Geological Society, February 22.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Mr. John Thornton Harrison, C.E., and Mr. M. Hawkins Johnson, were elected Fellows of the Society.—The following communications were read:—1. "On supposed Borings of Lithodomous Mollusca," by Sir W. C. Trevelyan, Bart., M.A., F.G.S. The author referred to Mr. Mackintosh's paper on this subject (*Q. J. G. S.* vol. xxv. p. 280), and stated his conviction, from examination of specimens, that the holes in question are the work of *Helices*, or other terrestrial Mollusca. He ascribed the same origin to the so-called "*Pholas*-borings" in the limestone at Orme's Head and elsewhere. He considered length of time to be a necessary element in the formation of these holes. The author also remarked that he had suggested a glacial origin for the terminal curvature of the laminæ of slate-rocks as early as 1849. Mr. Gwyn Jeffreys read extracts from a work published by the Rev. Mr. Hodgson in 1827, on the Natural History of Northumberland, in which these borings in limestone were referred to the action of snails. Mr. Jeffreys considered the foot to be the sole instrument employed by the boring Mollusca in excavating their burrows. He exhibited specimens of Lias from Lyme Regis perforated by *Pholas*, and of hard limestone from Malta perforated by *Lithodomus*, and remarked, in connection with the notion that asperities on the shell might be boring agents, that the shell of *Lithodomus* is perfectly smooth. Prof. Ramsay mentioned that he had seen *Helices* taken out of these holes at Tenby by Dr. Buckland, who believed that the snails effected the perforations by the agency of an acid. Mr. Charlesworth thought that if so much uncertainty could prevail upon such a subject, it threw great doubt upon some of the grandest generalisations of geology. He referred to the evidence connected with the glaciation of the Great Orme's Head, in which the origin of the perforations under discussion was of much importance, Mr. Darbyshire maintaining that they were the work of *Pholades*, while Mr. Bonney asserted that they were produced by snails. In the same way the origin of the celebrated borings in the Temple of Jupiter Serapis might be disputed, and the generalisation founded upon it rendered doubtful. Mr. Charlesworth noticed the necessarily small proportion of borers to the whole snail population of Britain, and remarked especially upon the absence of perforations in the chalk districts. He considered that repeated observations were necessary before this snail-engineering could be admitted, and suggested a systematic course of experiments. Mr. Boyd Dawkins suggested that the carbonic acid exhaled by snails in respiration might act upon limestones, and remarked that chalk weathers too rapidly to preserve the excavations.—2. "On the probable Cause, Date, and Duration of the Glacial Epoch of Geology," by Lieut.-Col. Drayson, R.A. In this paper the author started from the fact that the pole of the ecliptic could not be the centre of polar motion, as the pole varied in its distance from that centre. He indicated the curve which the pole did trace, and this curve was such as to give for the date 13,000 B.C. a climate very cold in winter, and very hot in summer for each hemisphere. The duration of the glacial epoch he fixed at about 16,000 years. The calculations resulting from this movement were stated to agree accurately with observation. Prof. Ramsay inquired whether the author's theory involved the recurrence of glacial epochs, and whether he considered the course of phenomena to be constant in early astronomical epochs. Rev. Osmond Fisher inquired whether the theory was founded on observed facts, or whether it was a purely physical theory. He also asked whether the line representing the change in the direction of the pole formed a re-entering curve, and whether the theory would account for the climate of Greenland in Miocene times. He suggested changes in the form of the earth which must have affected the direction of its axis. The President remarked upon the difficulty that arose from astronomical theories differing so much among themselves. He referred particularly to Adhémar's theory, and remarked that the difficulty connected with it is, that it invokes a recurrent cause, which must produce similar effects every 21,000 years, whilst there is very little evidence of glacial action during the whole long period of the Tertiary epoch. The author, in reply, stated that he could not go back beyond 30,000 years, but that he thought glacial conditions must recur. He had not astronomical data beyond 2,500 years, and these were very vague. The motion would be the same in kind, but uncertain in degree. His theory was based entirely upon observed facts. In laying down the curve, he considered it safe to go as far as the semicircle, as he had observations covering 40°; but he could not say whether the curve would be a re-entering one, although it showed a tendency that way, and would certainly be very nearly so. With regard to the change of climate of Greenland, as evidenced by its Miocene flora, he was not sufficiently versed in botany to pronounce an opinion. He remarked, in conclusion, that the distance of a planetary body from the sun did not seem to affect climate, and stated that Venus is at present suffering under a most severe glacial epoch.—3. "On Allophane and an allied Mineral found at Northampton," by Mr. W. D. Herman. In this paper the author gave analyses of an amorphous, translucent, reddish-yellow mineral, found incrusting sandstone in the ironstones of the Northampton sands, the comparison of which with Mr. Northcote's analysis of allophane from Charlton leads him to infer the identity of the two minerals. He also noticed a soft white substance found in certain joints in a section of the Northampton sand, and also referred to allophane by the late Dr. Berrell, who analysed it. This substance was said to occur not unfrequently in the inferior oolite of the Midland Counties. By analysis, it was shown to agree nearly with Samoite and Halloysite. Mr. David Forbes stated that he had found phosphoric acid in the first-mentioned mineral, which was perhaps the cause of its lustre. The mineral was probably not pure allophane. Prof. Morris suggested a chemical and microscopical examination of the strata above the places in which these minerals occur, which would probably reveal the conditions under which they have been formed. They were probably produced by the decomposition of silicates in the overlying rocks during the percolation of water. This applied also to the Charlton locality. Mr. Carruthers mentioned that allophane often fills the inflorescence of the Cycads of the Yorkshire Oolite, entirely destroying the vegetable structure, and that it also occurs in clay nodules from the coal-measures. Mr. Carruthers suggested that the decomposition of vegetable matter in clays might aid in the production of the mineral. —4. "Notes on the Peat and underlying Beds observed in the construction of the Albert Dock, Hull," by J. C. Hawkshaw, Esq., M.A., F.G.S. The Albert Dock is situated on the foreshore of the River Humber. The excavation for the dock extended over an area of about thirty acres, and they were carried down to a depth varying from 8 feet to 27 feet below low water of spring-tides. Beneath the more modern deposits of Humber silt a bed of peat, Hessle Clay, Hessle Sand, and purple clay, were successively met with. The peat was found at the west end of the Dock at the level of low water; at the east end the bed dipped so that the upper surface was found at eight feet below the level of low water. In the peat were found the remains of a fire, which the writer attributed to human agency. Oak-trees of large size were imbedded in the peat, some of which had grown where they were found, as was shown by the stools remaining with the roots penetrating the Boulder-clay beneath. In one oak-tree, five feet in diameter, a hole was found filled with acorns and nuts. Many of the nuts were broken open at the ends, and had evidently formed part of the store of a squirrel. Remains of Coleoptera were found, and one horn-core of a *Bos*. The excavation did not extend below the upper parts of the purple clay. Some of the borings, however, penetrated the chalk at a depth 85 feet below low water level, passing through a bed of sand 16 feet thick below the purple clay. Several thousand cubic yards of this sand were brought up into the foundations by springs of water which flowed up through old bore-holes. The abstraction of this sand from beneath the clay-bed caused it to subside many feet. The writer thinks that analogous subsidences may take place from natural causes; for instance, where large springs occur in tidal rivers. Two sections exhibited showed the beds above the chalk for a distance of rather more than a mile along the foreshore. The Hessle Sand was shown to thin out to the westward. It does not, in the writer's opinion, increase in thickness in that direction, as it was shown to do in a section already published in the Proceedings of the Society. The President remarked upon the singularity of the occurrence of a bed of ashes at such a depth in these deposits. Mr. Gwyn Jeffreys referred to the President's paper on the Kelsey Hill beds, and remarked on some of the mollusca obtained by Mr. Hawkshaw. Mr. Boyd Dawkins mentioned the occurrence of a submarine forest on the coast of Somersetshire, forming a layer of peat, beneath which was a land-surface, on which the forest had grown, and in which flint-flakes were found at Portlock and Watchet on digging through the peat. He remarked on the depression of the coast of Somersetshire within the human period, and suggested that the forest at Hull may have been contemporaneous with that of Somersetshire. Prof. Morris inquired whether any trees or roots were found as when growing. The shells obtained were estuarine. Prof. Morris remarked on a submerged forest near Whittlesey, with terrestrial plants and freshwater shells imbedded in the overlying clay. The author, in reply, stated that the trees had fallen where they grew. The general appearance of things led him to the belief that the fire which had destroyed part of the forest was of human production. The following specimens were submitted to the meeting:—Specimens of Allophane, from the Northampton Sands; exhibited by Mr. F. V. Runler, F.G.S. Specimens of Websterite, from the junction of the Tertiaries and the Chalk, near Bromley, Kent, exhibited by Mr. W. Whitaker, F.G.S. Specimens from the Peat-beds at the Hull Albert

Dock; exhibited by Mr. J. C. Hawkshaw, F.G.S., in illustration of his paper. Specimens of *Pholas*-borings from Lyme Regis, and of *Lithodomus*-borings from Malta; exhibited by Mr. J. Gwyn Jeffreys, F.R.S.

Chemical Society, March 2.—Prof. Williamson, F.R.S., president, in the chair. The following gentlemen were elected Fellows:—G. D. Harding, W. H. Hudleston, A. H. Mason, J. J. Nicolson. The following papers were read:—"On the distillation and boiling point of glycerin," by T. Bolas. It is known that when glycerin is heated under the ordinary atmospheric pressure so much as to cause ebullition it is more or less decomposed. This decomposition may be, however, prevented by a reduction of the pressure in the apparatus employed. The author has in that way found that pure glycerin boiled under a pressure of $12\cdot5^{mm}$ at 179·5° C and under a pressure of 50^{m} at about 210° C.—"On the action of Heat on Silver Nitrite," by Dr. E. Divers. The products of this action consist principally of silver nitrate, reguline silver, and oxides of nitrogen. But the relative proportions of the quantities of these substances to each other, and consequently the composition also of the gaseous matter, vary considerably in different experiments. When the nitrite is heated in an open vessel over a lamp or in an oven at any temperature between 85° and 140° C. the result of the operation may be represented by the equation $3\,NO_2\,Ag = N_2\,O_3 + Ag_2 + NO_3\,Ag$. When, instead of an open crucible, a closely covered one is used, so that the gaseous and fixed products of decomposition may be kept for a time in contact, the ultimate change effected in this way approaches, though not closely, to what is expressed by the equation $2\,NO_2\,Ag = NO + Ag + NO_3\,Ag$. In a third series of experiments, where the nitrite was heated in a vessel only nearly closed, the facts observed show that there is a tendency to yield only metallic silver and nitrogen peroxide, thus: $NO_2\,Ag = Ag + NO_2$. From all his experiments Dr. Divers draws the conclusion that like other silver salts the nitrite splits up under the influence of heat into metallic silver and the acid radical or its components, and that silver nitrate, nitric oxide, and, perhaps, nitrous anhydride are formed only by secondary reactions. The fusion which occurs in the mass of heated nitrite so soon as it has undergone some oxidation causes the author to throw out the suggestion that the nitrate formed perhaps combines with the nitrite to a nitrite-nitrate or hyponitrate.—After the reading of the above papers Dr. Gladstone communicated some remarks on the "Relations of Chemical Reaction and Time." He had instituted most varied experiments bearing on this subject, and, in briefly mentioning some of them, he wished to call the attention of chemists to this wide field of inquiry. Hitherto experimentators seemed to have limited their observations to only the circumstances at, and the products with, which a chemical reaction begins and ends, all that happens between was left wholly unnoticed. How fruitful attention paid to the intermediate products of a reaction could be is seen in the beautiful results which Prof. Williamson had gained on his researches on Etherification. The President, Dr. Odling, Mr. Vernon Harcourt, and others concurred in Dr. Gladstone's view as to the importance of a closer study of this subject.

Anthropological Institute of Great Britain and Ireland, March 6th.—Dr. R. S. Charnock, Vice-president, in the chair.—The following new members were elected:—Messrs. C. P. L. Naidoo Garroo, Henry Cook, Joseph Sharpe, LL.D., Danby P. Fry, Charles Edward Moore, Jesse Tagg; and W. S. W. Vaux, F.R.S., an honorary member.—Colonel Lane Fox exhibited a flint implement from Honduras.—Mr. Edward Blyth exhibited some cloth from West Africa.—Mr. Josiah D. Harris read a letter from his son on some remains found in the Macabi Islands, Peru.—Mr. J. W. Jackson read a paper "On the Racial Aspects of the Franco-Prussian War." After some remarks on the Aryan and Semitic divisions of the so-called Caucasian race, the former being defined as the flower of a Turanian, and the latter of a Negroid root, the author said that in the present imperfect state of our knowledge, it was impossible to decide whether Europe or Asia should be regarded as the primal and appropriate habitat of the Aryan, although he inclined to the former hypothesis. Neither could we yet assign the date when, and the place where, the various sub-divisions of this great race originated, and so must be contented with the fact of finding Slavons, Iberians, Teutons, and Celts on their existing areas of occupation, when, like the flora and fauna that accompany them, they must be regarded as Telluric organs. From a rapid survey of the earlier periods of European history, it was shown that the Celtic area of Gaul and Britain must have been ethnically effected at the time of the Roman conquest, which civilised but did not physically regenerate the Provincials. This was effete at the Gothic conquest of the Empire, when the Gauls received a slight and imperfect, and the Britons an effectual, baptism of Teutonic bone and muscle. The result of this diversity of fortune is seen in the fact that France, which retained more of the refinement, and with this more of the corruptions, of classic culture than Britain, preceded the latter in the attainment of civilisation, and now, after some centuries of quasi-imperial leadership in literature, science, manners and taste, is once again sinking into national weakness as an inevitable result of racial exhaustion. Hence it is that she no longer produces masterminds in any department, not even in war. Where are the successors of Cuvier and La Place, of Corneille, Racine, and Voltaire? This ethnic collapse of France, however, does not necessarily imply a subsidence of the entire Celtic area of Western Europe, as Britain is still at her maximum of racial vigour, and, like Rome after the decadence of Greece, will probably inherit that portion of the mission of imperial leadership forfeited by her effete sister and former rival. The Germans cannot do this, having so recently attained to unification, and being consequently devoid of any great capital like London, which may serve as the future metropolis of cultivation. Their mental constitution is, moreover, not adequately synthetic for the mission of Imperial centrality, which must accordingly devolve on England, the geographical terminus of the great north-western march of empire from the Euphrates to the Thames. Discussion having ensued, on the motion of Mr. Joseph Kaines, seconded by Capt. Pim, it was adjourned till the 20th instant.

Linnean Society, March 2.—Mr. G. Bentham, president, in the chair. The following papers were read:—On the Tamil Mames of Plants, by Rev. S. Mateer; Contributions towards a Knowledge of the *Curculionidæ*, by Mr. H. P. Pascoe.

Royal Institution of Great Britain, March 6.—Sir Henry Holland, Bart., M.D., F.R.S., president, in the chair. Messrs. W. Blenkin, J. Browning, E. Maynard Denny, F. A. Eck, Sir Frederick Elliot. K.C.M.G., Col. A. Lane Fox, Mr. P. Graham, Col. J. A. Grant, C.B., Mr. E. W. Grubbe, Dr. G. Harcourt, Capt. F. Helbert, Mr. G. W. Henderson, Mr. G. Middleton Keill, Dr. J. Kennedy, Mr. J. Macauley, Mr. K. R. Murchison, Mrs. Sheffield Neave, Mr. G. W. Royston Pigott, Mr. Eustratios Ralli, Mr. F. S. Reilly, Mr. W. C. Roberts, Mr. W. Dehague Routh, Mrs. W. C. Smith, Mr. T. Sowerby, were elected members.

MANCHESTER

Literary and Philosophical Society, February 7.—Mr. E. W. Binney, F.R.S., president, in the chair. "On the Organisation of an Undescribed Verticillate Strobilus from the Lower Coal Measures of Lancashire," by Professor W. C. Williamson, F.R.S., &c.—"The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena, part ii.," by Prof. Osborne Reynolds, M.A.—"Further Experiments on the Effects of Cold upon Cast Iron," by Peter Spence, F.C.S., &c.

February 21.—Mr. E. W. Binney, F.R.S., president, in the chair. "The Overthrow of the Science of Electro-Dynamics," by John Hopkinson, D.Sc. In science no theory should be considered unquestionable and no man's work held sacred from attack, and our scientific periodicals should afford the freest scope to discussions no matter how hostile to established notions. Still, it is evident that the journals ought not to publish everything that may come to hand; they should at least take care that a hostile critic understands the meaning of what he criticises. Two papers appeared last month in the *Quarterly Journal of Science* and the *Chemical News* respectively, in which the author (the Rev. Mr. Highton) somewhat summarily disposes of the science of Thermodynamics, fancying he has disproved the equivalence of heat and work. I will only trouble you with one or two quotations with a view to support my opinion that the papers in question ought never to have been permitted to appear in any journal pretending to scientific position. In the *Chemical News*, p. 42, we find, speaking of Joule and Scoresby's experiments on electro-dynamic engines—"They say that 'the quantities of zinc consumed' (that is, respectively, when the engine is at rest and doing work) 'being as a to b, $(a - b)$ represents the quantity of heat converted by the engine into useful mechanical effect.' Therefore, since on the supposition of a mechanical equivalent of heat a grain of zinc consumed equals 158 foot pounds, if x = pounds raised a foot high per consumption of a grain of zinc in the battery,—

$$x = \frac{(a - b)\ 158}{a}$$

Hence the authors draw the conclusion :—'Therefore when b vanishes, or becomes infinitely small, the economical duty is a maximum.' Certainly this is a most startling result; that the maximum of work should be done when no zinc at all is consumed." The last sentence is a mis-statement of the conclusions of Joule and Scoresby's paper, in which (*Philosophical Magazine*, vol. 28, p. 451) it is stated that "the economical duty will be a maximum when b vanishes or becomes infinitely small in comparison with a. In this case $x = 158$, while the power of the engine will become infinitely small with regard to work performed in a given time." Comparing the phrases "economical duty" and "maximum of work," as he uses them, he evidently confuses the duty of an engine with the whole work done by it. A little further on we have—"They calculate the maximum theoretical power of a grain of zinc to be 158 foot pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a - b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a - b)$, and it does not follow that $\frac{W}{(a - b)}$ with which we are concerned would be at all changed. What becomes then of the dogmatic assertion that the duty of a grain of zinc could be increased tenfold? Now let us turn to the paper in the *Quarterly Journal*. Here we may find enough in one article for our present purpose, taking chap. ii. art. 2,—"Why are we forced to suppose . . . . . . that the same amount of fuel produces the same amount of energy, whether it is consumed in the steam-engine, the horse . . . . . . the gnat? At any rate, we may observe that the very phrase is certainly a misnomer, and a misnomer of such a kind as to have a fatal effect in producing a false conception of things. For mechanical energy just as often produces cold as heat; it may produce either heat or cold, or neither. In fact, as a general rule, though with notable exceptions, every pushing or compressing force produces heat, and every pulling or expanding force cold. Place a weight on a pillar, and the weight produces heat in the pillar; hang it on a wire and it cools the wire. In exactly the same way, in a fire-syringe use force to press down the piston, it produces heat—heat enough to kindle tinder; but use the same force to pull up the piston, and it produces cold." Surely this is enough to show that the author's notions of what he is attacking are, to say the least of it, shallow; for what he quotes as paradoxes are simple deductions from the two laws of Thermodynamics. That a wire is cooled by stretching follows from the fact that heat expands it. In the case of the fire-syringe the case is simpler. The working body is the air in the syringe; on pulling up the piston this air does work, and therefore uses up heat and is cooled. Mr. Highton seems to imagine that because the arm of the experimenter does work, it is done *on the air in the syringe*, whereas this column of air and the observer are really co-workers in raising the air *external* to the cylinder. To point out all the fallacies of these papers in detail would take too much of your time. My object was to show that if the *Quarterly Journal of Science* and the *Chemical News* are to represent scientific opinion with any degree of truth, they would do well to use a little discretion as to what they print.—"Remarks on Mr. Spence's Experiments on the Effects of Cold on the strength of Cast Iron," by Joseph Baxendell, F.R.A.S. This was in reply to an assertion made by Mr. Spence the previous week that "he had so much confidence in the experiments then detailed, that he had no hesitation in giving it as an ascertained law that a specimen of cast iron having at 70° F. a given power of resistance to transverse strain will, on its temperature being reduced to zero, have that power increased by 3 per cent." Taking all the experiments on the effect of cold on iron which have yet been brought before the Society, they can only be regarded as indicating that if any effect at all is produced, it is more apparent on iron of good quality than on inferior iron, but that its amount is so small as to be wholly inadequate to account for the railway and other accidents which have been attributed to it.—"Further Observations on the Strength of Garden Nails," by J. P. Joule, F.R.S., &c. Since communicating the paper on the Alleged Influence of Cold in giving brittleness to Iron, I have collated the results with cast-iron nails in order to show the range of strength in such specimens:

Height of Fall of Hammer.	Percentage of Fractures.
2 inches	0
$2\frac{1}{2}$,,	0
3 ,,	6·25
$3\frac{1}{2}$,,	23·5
4 ,,	30
$4\frac{1}{2}$,,	36·4
$5\frac{1}{2}$,,	37·5
$6\frac{1}{2}$,,	48
7 ,,	65·5
$7\frac{1}{2}$,,	62·3
$8\frac{1}{2}$,,	75
10 ,,	92·8

I chose the garden nails for experiment after some thought, as presenting a marked variety of metal in contrast with the iron and steel wire, tempered and untempered. I did not expect them to possess great strength, but having found them to require a heavier blow than I expected to fracture them, I have had the curiosity to make some experiments on them which may be interesting to the Society. I took pairs of the nails, placed them head to point parallel to each other, so that pressure applied in the middle by pincers sufficiently forcibly would fracture one of them. Paper slips were pasted on the edges of the nails, and their distances asunder measured by a microscope with micrometer eyepiece divided by lines corresponding to $\frac{1}{800}$ of an inch. Weights were gradually added to the lever of one arm of the pincers until fracture took place, which was always accompanied with a sharp report. The observed deflection or bending of the nails was taken continuously as the weights were laid on, and the calculation of what it would have been at the moment of rupture taken from the immediately preceding observations. The amount of deflection was almost exactly proportional to the weight laid on in each experiment.

No. of Experiment.	Length of Nail between Supports.	Breadth of Nail in Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. lbs.
1	1·05	0·13	0·127	·0062	145·5
2	1·1	0·114	0·125	·0067	141
3	1·1	0·120	0·115	·0090	171
4	1·08	0·111	0·106	·0073	142·5
5	1·12	0·122	0·145	·0098	189
6	1·06	0·138	0·120	·0087	184·5
7	1·08	0·150	0·118	·0095	201
Average	1·084	0·1264	0·1223	·0082	167·8

If we compare the above with Mr. Brockbank's experiments, we shall find, approximately, on reducing them to the dimensions he adopted, viz., three feet between supports and one inch section:—

	Breaking weight.	Deflection.
Mr. Brockbank's, with large bars...	860·7	·740
My own, with nails	2673·	1·106

The metal, in the form I used it, was therefore more than three times as strong as that of the large bars to resist a compressing and tensile force, while its extent of spring at the breaking weight was half as much again. Therefore, so far from being of inferior quality, it would sustain a very much heavier blow without fracture.—"On the Action of Sulphurous Acid on Phosphates," by Dr. B. W. Gerland.

MAIDSTONE AND KENT

Natural History and Philosophical Society, Feb. 20.—An aggregate meeting of the members of the Maidstone and Mid Kent Natural History Society was held at the Charles Museum, when Dr. Monckton, one of the vice-presidents, delivered a lecture on "The Metaphysics of Zoology."

EDINBURGH

Royal Society, February 20.—Mr. W. F. Skene, V.P., in the chair. The following papers were communicated:—1. "On the Pentatonic character of Scottish Music," by the Hon. Lord Neaves. 2. "On the Motion of Solids in a Liquid," by Sir W. Thomson. 3. "Laboratory Notes on Thermo-electricity, and on Phyllotaxis," by Prof. Tait.

PARIS

Academy of Sciences, January 27.—M. Janssen sent to the French Institute a letter to explain how his expedition failed, owing to the persistency of the clouds. He acknowledges fully the kind proposition of the Organising Committee to take steps to procure a *laissez-passer* from M. Bismarck on his behalf. In five hours M. Janssen proceeded from Paris to the mouth of the Loire, where he landed on the 2nd December at 11 o'clock in the forenoon. The journey was magnificent. M. Janssen travelled at a height of 1,100 metres at the beginning, but after sunrise his balloon elevated itself through the warmth of the sun, and he reached 2,000 metres without throwing out any ballast. M. Janssen has invented an instrument for helping aëronauts in the determination of their way along the earth. This contrivance, which was highly praised by M. Dumas, will be fully described.—M. Bazin presented a projectile gun which explodes in the air, where it is sent by a larger gun remaining on the ground. This projectile gun, when exploding at a certain point of the *trajectoire*, sends an explosive bullet with a new impulse. It is impossible to claim precision for such a projectile, which is called very properly *a double effect*, but the distance to which it reaches is increased, and the final bullet was sent to 7,919 metres, it appears with some effect. The experiment was tried against the Prussians during the last days of the siege.

February 21.—The sitting was very thinly attended by members. The public was more numerous. No scientific paper had yet resumed its publication. M. Faye presided over the sitting. M. Elie Beaumont and M. Dumas sat on the platform, reading over alternately the correspondence. An article on the "Duquesne" Expedition was sent by M. de Fonvielle. M. Elie de Beaumont, who had predicted that the "Duquesne" would go to Switzerland on account of its directing power, opposed the reading.—M. Stanislaus Meunier read a paper on the nature of meteorites, which, he thought, are evidently of astral nature. He is opposed to the theory of Schiaparelli, who accounted for them by supposing they are in some respects allied to comets.—M Chevreuil read over a letter from M. Vaillant, who hopes to be soon enabled to resume his seat.—M. Delaunay read the translation of a letter received from Prof. Piazzi Smyth, the director of the Observatory at Edinburgh. He noticed the admirable working of the International French Telegraphy. That service was transferred to Tours and from Tours to Bordeaux, where it is now working under M. Marié Davy's superintendence.—M. Ch. Saint Claire Deville reported that Dr. Berigny of Versailles had not interrupted for a single day his admirable series of observations kept regularly for 24 years. M. Renan, who is living at Vendôme, did not interrupt his observations, although he was several times arrested as a spy, but he was saved by his thermometer! The maximum of cold has reached − 16° at Montpellier, − 17° at Bordeaux, and − 3° at Perigueux. The winter was a sharp one. It was predicted by M. Renan, as belonging to a series of recurring sharp winters. This law of recurrence was published in *Comptes Rendus* eighteen months before war broke out.—News was circulated of other learned men amongst the members. M. Martillet remains at St. Germain as curator of the Prehistoric Museum. One new hall was opened during the Prussian occupation, the Prussian Emperor, Princes, and generals frequently visiting the galleries. M. de Verneuil has established himself at Duc de Broglie's castle in Normandy. He is a relative of the present French ambassador, who is very much interested in scientific matters. M. Janssen is at Bordeaux, and expected daily by his family.

DIARY

THURSDAY, MARCH 9.

ROYAL SOCIETY, at 8 30.—Magnetic Observations made at Stonyhurst College Observatory, from 1863 to 1870: Rev. S. J. Perry.—Preliminary Notice on the Production of the Olefines from Paraffin by Distillation under Pressure: T. E. Thorpe and J. Young.—On the Action of Hydrobromic Acid on Codeia: Dr. C. R. A. Wright.

SOCIETY OF ANTIQUARIES, at 8.30.—On a probable allusion to the Chsritians in a passage of the Sixth Satire of Juvenal: Earl Stanhope, P.S.A.

LONDON MATHEMATICAL SOCIETY, at 8.—Remarks on the Mathematical Classification of Physical Quantities: Dr. Clerk Maxwell, F.R.S.—On Skew Cubics: Prof. H. J. S. Smith, F.R.S.—Note on the History of Certain Formulæ in Spherical Trigonometry: I Todhunter, F.R.S

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

LONDON INSTITUTION, at 7.30.—On the Colonial Question: Prof. J. E. Thorold Rogers, M.A.

FRIDAY, MARCH 10.

ROYAL ASTRONOMICAL SOCIETY, at 8.

QUEKETT MICROSCOPICAL CLUB, at 8.

ROYAL INSTITUTION, at 9.—The latest Scientific Researches in the Mediterranean and Straits of Gibraltar: Dr. W. B. Carpenter, F.R.S.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

SATURDAY, MARCH 11.

ROYAL SCHOOL OF MINES, at 8.—Geology: Dr. Cobbold, F.R.S. (Swiney Course.)

ROYAL INSTITUTION, at 3.—Spirit of the Age: Mr. O'Neil.

SUNDAY, MARCH 5.

SUNDAY LECTURE SOCIETY, at 3.30.—On Ferns: Dr. Cobbold, F.R.S.

MONDAY, MARCH 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—On Astronomy: R. A. Proctor, F.R A.S. (Educational Course.)

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

TUESDAY, MARCH 14.

PHOTOGRAPHIC SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.

WEDNESDAY, MARCH 15.

SOCIETY OF ARTS, at 8.—On the Different Methods of Extracting Sugar from Beet-root and Cane: Ferdinand Kohn.

METEOROLOGICAL SOCIETY, at 7.—Evaporation, Rainfall, and Elastic Force of Vapour: J. R. Mann.

ROYAL SOCIETY OF LITERATURE, at 8.30.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Teeth of Mammalia: Prof. Flower.

THURSDAY, MARCH 16.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

LINNEAN SOCIETY, at 8.

CHEMICAL SOCIETY, at 8.

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